anatomical and histological evidence summarised in my recent paper—will now see that unless they can disprove the statements of Prof. Marshall, Dr. Jickeli, Dr. P. Herbert Carpenter, and myself, they are bound to admit my doctrine, and to show how their theoretical homology is to be reconciled with it.

WILLIAM B. CARPENTER

56, Regent's Park Road, London, N.W., November 3

Natural Science for Schools

THE thoughtful and suggestive paper of Prof. Armstrong in the last number of NATURE (p. 19) is to be commended to the attention both of science teachers and of the head masters of our schools. It is undoubtedly true that, with few exceptions, science is still either completely neglected by our schools or handled in a way which does not at all tend to advance its interests. When it is made a "refuge for the destitute," or considered only fit for those intellectually unequal to the study of classics and mathematics, no wonder that observant head masters conclude that little good is to be got from it.

As a science master of many years' experience (having been in fact responsible for the introduction of science into *two* of the schools named by Prof. Armstrong as exceptions to the universal indifference), you will perhaps allow me to call attention to the importance of Prof. Armstrong's paper, and to give the conclusions to which my own experience has led me.

The importance of clearly understanding the purpose with which science is to be studied, and the distinction to be borne in mind between the best curriculum for those who are to be professed chemists and those who will not carry the study of chemistry beyond their school-days is obvious; but I wish to point out how entirely science masters are at the mercy of examiners, both of University examiners, periodically examining a school, and of examiners for open scholarships. My own experience is to the point. Fully persuaded of the uselessness of attempting to make an analytical machine out of the ordinary school-boy giving two or three hours a week to chemistry for two or three years, and of the very small amount of education to be obtained from such a course, I endeavoured to model my instruction in practical chemistry much upon the lines adopted by Prof. Armstrong, and exemplified in the appendix to his paper. When the examinations came, it was duly explained to the examiner that the course of instruction adopted had been unusual, but, all the same, the papers set were of the usual kind:—"Analyse the mixture A," "Determine the metals and acids present in the solution B," &c. On such a paper, of course, the boys failed, and a depreciatory report was sent up by the examiners, with the result that the governors of the school thought it their duty to interfere, and request that "more attention should be given to practical chemistry." Consequently my attempt had to be abandoned, and we returned to our "test-tubing."

Scholarship examinations, being presumably of those who will carry the study much further, may more reasonably demand a knowledge of the ordinary methods of analysis, but I am glad to see that a considerable change has taken place in the papers set, and that now the questions proposed are often such as to place the mechanical analyst at a disadvantage, and to encourage the intelligent observation and interpretation of phenomena.

Prof. Armstrong of course writes as a chemist. But there can be no doubt that certain portions of physics are educationally more useful, and it seems to be only the difficulty of arranging practical work in physics which has led to the present state of things, where practical science work in schools means nearly always practical chemistry. But Prof. Armstrong's protest against allowing this to degenerate into "test-tubing" should not be disregarded. There seems also no reason why *elementary* instruction in science—whether chemistry, or botany, or physiography—should not deal *first* with the familiar things of every-day life. I think much more training is to be got by determining, as Prof. Armstrong suggests, the composition of air, the relative combining weights of silver and lead, &c., than by seeing made any number of oxides of nitrogen, and listening to a description of their properties. There is, however, considerable difficulty in arranging easy methods of determining chemical equivalents which, in inexperienced hands, shall give results not *too wide* of the mark.

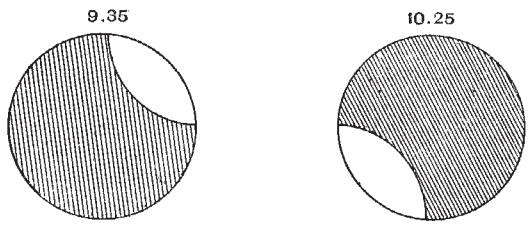
If a boy gets out the atomic weight of oxygen as 9 when the book says it is 16, or finds the latent heat of steam to be 300 and something when it ought to be 536, he begins to disbelieve in the precision of the statements made, and it is unfortunately impossible for a beginner to make *accurate* determinations of combining weights. Less erratic results can, in fact, be obtained in certain selected physical measurements.

The "bareness" of printed instructions is, as Prof. Armstrong remarks, a distinct advantage to the good student, by compelling him to think for himself, but it is fatal to the unintelligent student, to whom "thinking" is the very hardest work he is called upon to do.

SCIENCE MASTER

The Recent Lunar Eclipse

My object in writing is to confirm in some degree the peculiar appearance of the disk, noticed in your last number (vol. xxx. p. 632). The eclipse was seen here under the most favourable circumstances: the obscuration was so great that the disk could barely be discerned with the naked eye, and the copper colour usually seen was not noticed. Having watched the moon well into the umbra, my attention was diverted for a while, but, on looking again, at 9.35 G.M.T., I was surprised to see a portion of the north-east quadrant pretty strongly illuminated; my attention was again diverted, but on looking a second time at 10.35 G.M.T., I observed a portion of the south-east quadrant



illuminated in a somewhat similar manner. At both times the moon was well within the geometrical umbra. But the remarkable feature was that on both occasions the boundaries of the illuminated portions were, approximately, circular, and convex toward the axis of the umbra, indicating that the refracted solar rays producing these illuminations had crossed the axis of the shadow-cone previous to impinging on the lunar disk. The portions of the refracting annulus of the earth's atmosphere concerned in producing these effects were those superincumbent on the Southern Indian Ocean and the North Atlantic.

WENTWORTH ERCK

Shankill, Co. Dublin, November 4

The Sky-Glows

IN using the word "corona" to designate the coloured glare which has accompanied the sun during the past year, I had no intention of employing it in its astronomical sense, but in its ordinary meteorological meaning—which "G. M. H." (NATURE, vol. xxx. p. 633) has overlooked—as referring to the coloured circles on cloud and haze frequently to be seen round the sun and moon, and classed by some observers with halos. By calling the circle now visible round the sun a "corona," I mean that in appearance and probable optical cause it is more like a meteorological corona than like a halo.

May I be allowed to point out a misprint in the first paragraph of my last letter (vol. xxx. p. 633), where it should read "unusual sky phenomena"—the world *unwersal* having been printed for *unusual*.

T. W. BACKHOUSE

Sunderland, November 8

AFTER sunset this evening there was a peculiar pink flush in the western sky here similar to that which attracted so much attention in England last year. Twenty-five minutes after the sun had gone down, the colour was so vivid as to be reflected from the snows of Mount Baker (10,700 feet), which is about seventy-five miles east of this place. Shortly afterwards it disappeared, but reappeared thirty-five minutes later, prolonging the twilight and making the stars look green, finally dying away very gradually. The weather for the past twelve days has been very wet, and to-night's is the first clear sunset in that time.

Fourteen days ago, when on the Fraser River, eighty miles from here, I saw after sunset a very brilliant aurora borealis. I write this thinking there may be a repetition of the phenomena in England, in which case this note may possess interest.

G. W. LAMPLUGH

Victoria, Vancouver Island, October 13

Peculiar Ice Forms

THE ice structures observed by Mr. Woodd Smith (November 6, p. 5) are evidently the same as were described in vol. xxi. p. 396. I have often seen such fibrous masses since, under circumstances which left no doubt of their being mainly due to prolonged condensation of aqueous vapour from the air; the fibres, white like asbestos, and covered only by a very thin layer of earthy particles, rising from a hard subsoil. The absorption of aqueous vapour by the soil, especially on mountains, seems not yet to be duly appreciated, although it is proved by the many springs issuing at short distances below the summits, and has been insisted upon already in Er. Darwin's "Botan. Garden" and "Phytonomia" (chap. xi. 2). "Rainfall being the source of all water-supply" (NATURE, vol. xxx. p. 375) is a statement hardly to be maintained. W.

Freiburg, Badenia, November 8

Seismographs—An Apology

I AM just in receipt of the inclosed letter from Mr. Charles A. Stevenson, in which he claims the original idea of the actuating mechanism in the *horizontal component seismograph* I have lately described in these pages, and he includes a copy of his paper to justify his remarks. I therefore think it my duty to offer my apologies to him for not having given him full credit for his invention so far as it goes, although I have *unconsciously* done him wrong. Naples is unfortunately very badly off for modern scientific works and *Proceedings* of Societies, both as regards the National and the University libraries, and as far as I know no copy of Mr. Stevenson's paper exists in the town, except the one he has now sent me.

Perhaps I may be permitted to point out that Mr. Stevenson's seismograph, so far as it is described, would be almost useless for the following reasons:—

(1) The inertia of the upper glass plate would be insufficient not to be affected by the slight movement conducted through the ivory balls to it. This is the reason I use the very heavy lead disk.

(2) No earthquake shock is perfectly horizontal, so that Mr. Stevenson's instrument would only be fit to register the horizontal component of the earth-wave, and would fail to do this, since if the angle of emergence was appreciable it would be jerked up off its supports, and consequently would simply register a series of interrupted lines. This is why I introduced the upper balls and resistance plate.

(3) If the instrument was disturbed by an earth-wave of large amplitude, the registering arm would pass beyond the border of the smoked plate (unless the apparatus was of very great dimensions, so failing to fulfil the conditions of the British Association), where the needle would drop out, or fall so low as to prevent the return of the arm over the plate.

(4) If the earthquake was of some seconds' duration and composed of many varying movements, as is generally if not always the case, a network of irregular curves would remain on the glass that would be quite unintelligible.

If a thing is to be done, it is advisable to do it well, and it is less possible to have accurate registers of earthquake shocks than of the force and direction of the wind, barometric pressure, or any other meteorological phenomena. The requirements of the British Association with regard to expense, size, and portability of seismographs, will not permit anything like an accurate investigation of geodynamics.

In conclusion, should I have overlooked and appropriated the ideas of any other inventor, I shall be happy to fully acknowledge them if sufficient evidence is given (as in the above case) of priority of publication. H. J. JOHNSTON-LAVIS

November 7

45, *Melville Street, Edinburgh, November* 3

I NOTICED recently in NATURE (vol. xxx. p. 608) an article by you in which you describe a seismograph for recording earthquake shocks, which would appear to be your own invention from reading the paper. No doubt the method of making the record, springs, and upper balls are your own invention, but the *principle* on which the seismograph there described acts is, as far as I know, mine or my father's. I inclose the paper in which it was first described, and I would be glad to learn from you if you forestalled me. CHARLES A. STEVENSON

Dr. Johnston-Lavis, Naples

Fly-Maggots Feeding on Caterpillars

A FEW months ago I had a caterpillar of *Papilio erythronius*, which I found on a lemon-tree. I put it into a card-box, and fed it daily on lemon-leaves. The box was covered with cloth *tied tightly all round the opening*. After some days, the caterpillar fixed itself to the side of the box, and turned into a chrysalis in the usual way. One day on opening the box, instead of finding the chrysalis changing into its usual colours and markings, it was dark all over. A few days more, on re-opening the box, I found six fully-developed cream-coloured maggots at the bottom of the box. I was rather puzzled to conjecture how these maggots got into a box three inches high, with a bit of cloth tied all round the opening. I put the maggots into a little box with some earth under a tumbler. They immediately buried themselves in the earth. In a few days I found six chrysalides, and some days later there were six ordinary house-flies buzzing within the tumbler. I then examined the dark chrysalis of the *P. erythronius*, which was evidently dead, and found it only a *shell*. All its interior had been consumed by the six maggots. It is evident that these maggots in their infant stage had already been in the body of the caterpillar when I boxed it. The latter had gone through its transformation as if nothing was the matter with it, although, if one could have interrogated it, probably it would have complained of mysterious gnawings and creepings in its interior. A time, of course, came when, for want of nerve-centres and other organs, the chrysalis could not go on with its development into the perfect Papilio. The six maggots having had a full meal, found their way out of the Papilio's chrysalis in order to undergo *their* transformation.

I knew that the larvæ of the Ichneumonidæ fed on the live bodies of caterpillars, but I did not know that the larvæ of the house-fly did so also. E. BONAVIA

Etawah, India, October 18

THE CRYSTALLINE ROCKS OF THE SCOTTISH HIGHLANDS

EVER since the discovery of Silurian fossils in the rocks of North-West Sutherland, it has been recognised that in that region lies the key to the structure of the Scottish Highlands. Accordingly, when in the progress of the Geological Survey, the mapping of the Highlands had to be undertaken, I determined that a detailed survey of the Sutherland ground on the scale of six inches to a mile should be made as a basis for the work. In the summer of last year a surveying party under the charge of Mr. B. N. Peach was stationed there, with instructions to begin by mapping the Durness Basin. This duty was satisfactorily accomplished before the end of the season. The Silurian series of Durness was ascertained to be about 2000 feet thick, and to consist of numerous successive zones, which were traced on the six-inch maps and discriminated in such a way as to be recognisable should they be found to occur in the more complicated region to the east. With this necessary groundwork well established, the Eriboll tract was attacked this summer by Messrs. Peach and Horne. I had never myself had an opportunity of studying the Eriboll sections, which, from the days of Macculloch down to the present time, have been such a fruitful subject of discussion. It was a special injunction to the officers now intrusted with the detailed survey of the region to divest themselves of any prepossessions in favour of published views and to map the actual facts in entire disregard of theory. By the close of this last season the structure of the Eriboll area had likewise been traced upon the six-inch maps, and I then went north to inspect the work. From time